Mathiness in the Theory of Economic Growth

By PAUL M. ROMER*

Politics does not lead to a broadly shared consensus. It has to yield a decision whether or not a consensus prevails. As a result, political institutions create incentives for participants to exaggerate disagreements between factions. Words that are evocative and ambiguous better serve factional interests than words that are analytical and precise.

Science is a process that does lead to a broadly shared consensus. It is arguably the only social process that does. Consensus forms around theoretical and empirical statements that are true. In making these statements, a combination of words from natural language and tightly linked symbols from the formal language of mathematics encourages the use of words that are analytical and precise.

Economists mostly stick to science. Robert Solow (1956) was engaged in science when he developed his mathematical theory of growth. But they can get drawn into academic politics. Joan Robinson (1956) was engaged in academic politics when she waged her campaign against capital and the aggregate production function.

Academic politics, like any politics, is better served by words that are evocative and ambiguous, but if an argument is transparently political, economists interested in science will simply ignore it. The style that I am calling mathiness lets academic politics masquerade as science. Like mathematical theory, mathiness uses a mixture of words and symbols, but instead of making tight links, it leaves ample room for slippage between statements in the languages of words as opposed to symbols, and between statements with theoretical as opposed to empirical content.

Solow's mathematical theory of growth mapped the word "capital" onto a variable in the mathematical equations, and onto both data from national income accounts and things like machines or structures that we could observe. The tight connection between the word and the equations gave the word a precise meaning that facilitated equally tight connection between its theoretical and empirical claims. Gary Becker's mathematical theory of wages (1962) gave the words "human capital" the same precision and established the same two types of tight connections – between words and math and between theory and evidence, and the same microeconomic foundation in data and observations.

In contrast, McGrattan and Prescott (2010) give a label – "location" – to their proposed new input in production, but their mathiness leaves so much slippage that the word could mean anything. The authors choose a word that had already been given a precise meaning by mathematical theories of product differentiation and economic geography, perhaps to give an impression of meaning by association. However, the formal equations are completely different, so neither of those meanings carry over.

Their mathiness also offers no connection between its theoretical and empirical statements. The quantity of location has no unit of measurement. The term does not refer to anything a person could observe. In a striking (but instructive) use of slippage between the theoretical and the empirical, the authors assert, with no explanation, that the national supply of location is proportional to the number of residents. This raises questions that the equations of the model do not address. If the dependency ratio and population increase, holding the number of working age adults and the supply of labor constant, what mechanism leads to an increase in output? Does an additional 70 year-old retiree increase location by the same amount as an additional 5 year-old?

Their paper is one of several that introduce mathiness into growth theory to support price-taking and oppose monopolistic competition. In one sign that this campaign is political (in the sense of academic politics) rather than scientific, proponents offer neither theory nor ev-

* Romer: Stern School of Business, New York University, 44 W. 4th St, New York, NY 10012, promer@stern.nyu.edu. An appendix with computations and other supporting detail is available from the author's website and from the jounral website. The author acknowledges the generous support of the Rockefellar Foundation.

idence to support the trade-offs that this requires. For example, McGrattan and Prescott do not explain why price taking with no micro-foundation is better than market power with a micro-foundation.

For roughly two decades, growth theory has made no progress toward a consensus about the foundations for an economics of ideas. A stalemate prevails between Marshallian external increasing returns and monopolistic competition. One unfortunate side effect has been slow adoption in aggregate theory of a powerful abstraction from public finance – nonrivalry – which is far more consequential than the relatively minor differences implied by price-taking versus price-setting.

If mathiness is used infrequently to slow convergence to a new scientific consensus, it will do localized, temporary damage. Unfortunately, the market for lemons tells us that as the quantity of mathiness increases, it could do permanent, pervasive damage in economics. It is hard for readers to distinguish mathiness from mathematical theory.

The market for mathematical theory can survive a few lemon articles filled with mathiness. Readers will put a small discount on any article with math, but will still find it worth their while to work through and verify that the formal arguments are correct, that the connection between the symbols and the words is tight, and that the theoretical concepts have implications for measurement and observation. But after readers have been burned too often by mathiness that wastes their time, they will stop taking seriously any paper that contains mathematical symbols. In response, authors will stop doing the hard work that it takes to supply real mathematical theory. If no one is putting in the work to distinguish between mathiness and mathematical theory, why not cut a few corners and take advantage of the slippage that mathiness allows? The market for mathematical theory will collapse. Only mathiness will be left. It will be worth little, but cheap to produce and might survive as entertainment.

Economists have a collective stake in flushing mathiness out into the open. We will make faster scientific progress if we can continue to rely on the clarity and precision that math brings to our shared vocabulary, and if, in our analysis of data and observations, we keep using and refining the powerful abstractions that mathematical theory highlights – abstractions like physical capital, human capital, and nonrivalry.

I. Scale Effects

In 1970, there were zero mobile phones. Today, there are more than 6 billion. This is the kind of development that a theory of growth should address, but to be able to do so, the theory must accomodate scale effects.

Let q stand for individual consumption of mobile phone services. For $a \in [0, 1]$, let $p = D(q) = q^{-a}$ be the inverse individual demand curve with all-other-goods as numeraire. Let N denote the number of people in the market. Once the design for a mobile phone exists, let the inverse supply curve for an aggregate quantity $Q = qN$ take the form $p = S(Q) = Q^b$ for $b \in [0, \infty]$.

If the price and quantity of mobile phones are determined by equating $D(q) = m * S(Nq)$, so that $m \geq 1$ captures any mark-up of price relative to marginal cost, the surplus S created by the discovery of mobile telephony takes the form

$$S = C(a, b, m) * N^{\frac{a(1+b)}{a+b}},$$

where $C(a, b, m)$ is a messy algebraic expression. Surplus scales as N to a power between a and 1. If $b = 0$, so that the supply curve for the devices is horizontal, surplus scales linearly in N. If, in addition, $a = \frac{1}{2}$, the expression for surplus simplifies to

$$S = \frac{2m - 1}{m^2} N.$$

With these parameters, a tax or a monopoly markup that increases m from 1 to 2 causes S to change by the factor 0.75. An increase in N from something like 10^2 people in a village to 10^{10} people in a connected global market causes S to change by the factor 10^8.

Effects this big tend to focus the mind.

II. The Fork in Growth Theory

The traditional way to include a scale effect in a growth model was proposed by Marshall (1980). One writes the production of telephone services at each of a large number of firms in an industry as $g(X)f(x)$ where the list x contains the inputs that the firm controls and X is the list of inputs for the entire industry. One obvious problem with this approach is that it offers no basis for determining the extent is of the spillover benefits from the term $g(X)$. Do they require face-to-face interaction? Production in the same city, the same country, or anywhere?

If we split $x = (a, z)$ into a nonrival input a and rival inputs z and write output at an individual firm as $Af(a, z)$, a standard replication argument implies that f must be homogeneous of degree 1 in the rival goods z. (If the aggregate supply curve of goods is upward sloping, as in the example above, this is merely a sign that some of the factors in the list z are in fixed supply. Anything that looks like a "Marshallian rent" in a partial equilibrium analysis is in fact compensation to one of these fixed factors.) In a competitive equilibrium, for each firm, the value of output equals the compensation paid to the rival inputs z so there can be no nonrival inputs a that an individual firm can use whilst excluding other firms from using them. (For an elaboration of this argument, see Romer 1994b.) Hence, the nonrival inputs A must be 0% excludable and output must take the form $Af(z)$. No firm or person can keep a nonrival idea secret. No firm or person will have any incentive to take advantage of the surplus noted above by encouraging the diffusion of an idea like mobile telephony throughout the world. Discovery and diffusion have to happen by accident.

I started with the framework of external increasing returns in my Ph.D. thesis, but soon switched to a model with monopolistic competition that allows for nonrival inputs in production that could be at least partially excludable. In models with partially excludable nonrival ideas, it is logically possible for a firm to have an incentive to discover new ideas (Romer 1990) or to encourage the international diffusion of an existing idea like mobile telephony (Romer 1994a). In this framework, excludability offers a much more precise way to think about spillovers. Nonrivalry, which is logically independent, is the defining characteristic of an idea and the source of the scale effects that are central to any plausible explanation of the broad sweep of human history (Jones and Romer 2010).

As part of the campaign of academic politics noted above, economists commitment to price-taking persevered with the assumption of 0% excludability required for external increasing returns, even when this forces mechanical models in which agents have no incentive either to discover ideas or encourage their diffusion. To defend this approach, they too resorted to mathiness.

III. Examples of Mathiness

The McGrattan and Prescott article links a word with no meaning to new mathematical results. The mathiness in "Perfectly Competitive Innovation," (Boldrin and Levine 2008) takes the adjectives from their title, which have a well established, tight connection to existing mathematical results, and links them to a diametrically opposed set of mathematical results. In an initial period, the innovator in their model is a monopolist, the sole supplier of a newly developed good. Nevertheless, the authors force the monopolist to take a specific price for its own good as given by imposing price-taking as an assumption about behavior.

Boldrin and Levine (2008) also make free-standing verbal assertions (e.g. concerning Marshallian rents) that seem to invoke known mathematical results but which are in fact false. If they had written down a formal mathematical argument that was tightly linked to their words, they would have caught their error. In "Ideas and Growth," Robert Lucas makes a similar verbal claim that seems to invoke known results and likewise turns out to be false: "Some knowledge can be 'embodied' in books, blueprints, machines, or other kinds of physical capital, and we know how to introduce capital into a growth model, but we also know that doing so does not by itself provide an engine of sustained growth" (Lucas 2009, p. 6). Any model of growth with a growing variety of capital goods or a quality ladder of capital goods is a counter-example.

In Lucas and Moll (2014), the mathiness involves both words that misrepresent the mathematical analysis and a mathematical model that is not well specified. The authors develop a model based on an assumption that I'll call P (for their use of "Pareto.") They show that given P, the growth rate $g[P](t)$ converges to $\gamma > 0$ as t goes to infinity. Because P is hard to justify, the authors offer "an alternative interpretation that we argue is observationally equivalent: knowledge at time 0 is bounded but new knowledge arrives at arbitrarily low frequency." (Lucas and Moll 2014, p. 11.)

Let β denote this arrival rate for new knowledge. In their alternative interpretation, the authors consider a collection of economies that start under assumption B, but eventually switch to an economy where assumption P applies instead. As β gets arbitrarily low, the time it takes to switch to P goes to infinity. (See the online appendix for details.) Let $\beta : B \Rightarrow P$ denote one economy from this collection specified by the value β. Because any $\beta : B \Rightarrow P$ economy will eventually switch to being a P economy, the limit of the growth rate in every $\beta : B \Rightarrow P$ economy is equal to the limit of the growth rate in the P economy.

Yet at any date T that is large enough so that we can ignore transitory effects, $g[\beta : B \Rightarrow P](T)$ approaches 0 as β becomes arbitrarily low yet $g[P](t)$ is close to γ. Because any observation on the growth rate has to be taken at some specific date, all observations show that instead of being equivalent, the collection of economies $\{\beta : B \Rightarrow P\}$ differs markedly from the P economy.

The mathiness here involves more than a nonstandard interpretation of the phrase "observationally equivalent." The underlying formal result is that calculating the double limit in one order $\lim_{\beta\to 0}(\lim_{T\to\infty} g[\beta : B \Rightarrow P])$ yields one answer, γ, but calculating it the other, $\lim_{T\to\infty}(\lim_{\beta\to 0} g[\beta : B \Rightarrow P])$, gives a different answer, $0 < \gamma$. The mathiness here involves picking the calculation that is convenient and acting as if the double limit exists. An argument that takes the math seriously would note that the double limit does not exist and would caution against trying to give an interpretation to value calculated using one order as opposed to the other.

IV. A New Equilibrium in the Market for Mathematical Economics

As is noted in an addendum, Lucas (2009) contains a flaw in a proof. The proof requires that a fraction $\frac{\alpha}{\gamma}$ be less than 1. The same page has an expression for γ, $\gamma = \alpha\frac{\gamma}{\gamma+\delta}$, and because α, γ, and δ are all positive, it implies that $\frac{\alpha}{\gamma}$ is greater than 1. Anyone who does math knows that it is distressingly easy to make an oversight like this. It is not a sign of mathiness by the author. But the fact that this oversight was not picked up at the working paper stage or in the process leading up to publication may tell us something about the new equilibrium in economics. Neither colleagues who read working papers, nor reviewers, nor journal editors, are paying attention to the math.

I, and others, told Lucas and Moll about the discontinuity in the limit in their joint paper and the problem this posed for their claim about observational equivalence. I thought that publishing the paper in this form would be embarrassing for them. They kept this analysis in the paper and the Journal of Political Economy published it. This may reflect a judgment by the authors and the editors that at least in the theory of growth, we are already in a new equilibrium in economics in which readers have come to expect and accept mathiness.

One final bit of evidence comes from Piketty and Zucman (2014), which cites a result from a growth model: with a fixed saving rate, when the growth rate falls by half, the ratio of wealth to income doubles. They note that their formula $W/Y = s/g$ assumes that national income and the saving rate s are both measured net of depreciation. They observe that the formula has to be modified to $W/Y = s/(g+\delta)$, with a depreciation rate δ, when it is stated in terms of the gross saving rate and gross national income.

From Krusell and Smith (2014), I learned more about this calculation. If the growth rate falls and the **net** saving rate remains constant, the **gross** saving rate has to increase. For example, with a fixed net saving rate of 10% and a depreciation rate of 3%, a reduction in the growth rate from 3% to 1.5% implies an increase in the gross saving rate from 18% to 25%. This means that the expression $s/(g+\delta)$ increases by a factor 1.33 because of the direct effect of the fall in g and by a factor 1.38 because of the induced change in s. A third factor, equal to 1.09, arises because the fall in g also increases the ratio of gross income to net income. These three factors, which when multiplied equal 2, decompose the change in W/Y calculated in net terms into equivalent changes for a model with variables measured in gross terms.

Piketty and Zucman (2014) present their data and empirical analysis with admirable clarity and precision. In choosing to present the theory in less detail, they too may have been responding to the expectations in the new equilibrium. Empirical work is science; theory is entertainment. Presenting a model is like doing a card trick. Everybody knows that there is will be some sleight of hand. There is no intent to deceive because no one takes it seriously. Perhaps the norm will soon be like the one in professional magic; it will be impolite, perhaps even an ethical breach, to reveal how someone's trick works.

When I learned mathematical economics, a different equilibrium prevailed. Not universally, but much more so than today, when economic theorists used math to explore abstractions, it was a point of pride to do so with clarity, precision, and rigor. Then too, a faction like Robinson's that risked losing a battle might resort to mathiness as a last-ditch defense, but doing so carried a risk. Reputations suffered.

If we have already reached the lemons market equilibrium where only mathiness is on offer, future generations of economists will suffer. After all, how would Piketty and Zucman have organized their look at history without access to the abstraction we know as capital? Where would we be now if Robert Solow's math had been swamped by Joan Robinson's mathiness?

REFERENCES

Becker, Gary S. 1962. "Investment in Human Capital: A Theoretical Analysis." *Journal of Political Economy*, 70(5): 9–49.

Boldrin, Michele, and David K. Levine. 2008. "Perfectly competitive innovation." *Journal of Monetary Economics*, 55(3): 435–453.

Jones, Charles I., and Paul M. Romer. 2010. "The New Kaldor Facts: Ideas, Institutions, Population, and Human Capital." *American Economic Journal: Macroeconomics*, 2(1): 224–245.

Krusell, Per, and Anthony A. Smith. 2014. "Is Piketty's Second Law of Capitalism Fundamental." Yale.

Lucas, Jr., Robert E. 2009. "Ideas and Growth." *Economica*, 76(301): 1–19.

Lucas, Jr., Robert E., and Benjamin Moll. 2014. "Knowledge Growth and the Allocation of Time." *Journal of Political Economy*, 122(1): 1–51.

Marshall, Alfred. 1890. *Principles of economics.* London:Macmillan and Co.

McGrattan, Ellen R., and Edward C. Prescott. 2010. "Technology Capital and the US Current Account." *The American Economic Review*, 100(4): 1493–1522.

Piketty, Thomas, and Gabriel Zucman. 2014. "Capital is Back: Wealth-Income Ratios in Rich Countries 1700-2010." *The Quarterly Journal of Economics*, 129(3): 1255–1310.

Robinson, Joan. 1956. *The Accumulation of Capital.* Homewood, Illinois:Richard D. Irwin.

Romer, Paul M. 1990. "Endogenous Technological Change." *Journal of Political Economy*, 98(5): S71–S102.

Romer, Paul M. 1994*a*. "New Goods, Old Theory, and the Welfare Costs of Trade Restrictions." *Journal of Development Economics*, 43: 5–38.

Romer, Paul M. 1994*b*. "The Origins of Endogenous Growth." *Journal of Economic Perspectives*, 8: 3–22.

Solow, Robert M. 1956. "A Contribution to the Theory of Economic Growth." *Quarterly Journal of Economics*, 70(1): 65–94.